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Biomedical information, peer review, and conflict of interest as they influence public health.
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ABSTRACT: Peer review is the main quality-control mechanism in medical and scientific publishing. In the process of peer review, a research paper that has been submitted to a journal for publication is reviewed by an independent expert in the field. This expert evaluates the validity, reliability, and importance of the results and recommends that the manuscript either be accepted, revised, or rejected. The peer review process has failed to maintain the quality of research reports in some recent cases, and this is detrimental to the understanding of scientific issues as well as to the public's opinion of the scientific community. It is time for a thorough examination of the strengths and weaknesses of the peer review process. The public has a right to learn the results of such an evaluation because peer review is closely related to the expenditure of public funds for research activities. A case is discussed in which the peer review process became inappropriately involved in issues of "data ownership". A coinvestigator disagreed with the conclusions of the principal investigator. The latter researcher suppressed the opinions of the former and told the medical journal that the coinvestigator was not authorized to utilize the research results. In response, the journal editor ignored the dissenting views of the coinvestigator and also failed to submit them to the peer reviewers. This case demonstrates that dissent among authors, editors and reviewers may cause the peer review system to break down. It is vital that strategies for focusing on scientific truths, rather than data ownership, be developed for such situations. (Consumer Summary produced by Reliance Medical Information, Inc.)

TEXT:

Biomedical Information, Peer Review, and Conflict of Interest as They Influence Public Health The peer review process serves a vital role in the publication of biomedical information. When properly functioning, review should focus exclusively on questions of scientific validity and should avoid becoming enmeshed in questions such as "authorization" and "data ownership." We present a case in which the dissenting views of a coinvestigator were suppressed because the principal investigator and grantee institution informed a medical journal that the coinvestigator was not "authorized" to use the data generated by a publicly funded grant and because the editor of a scholarly journal refused to review the dissenting manuscript and to submit that dissent to external reviewers for peer review. The current peer review system, as shown by this case, is unable to embrace dissent within the peer review process and to use dissent to serve scientific truth and the public interest.

THE PEER review process is the traditional method of quality control in the scientific enterprise. Any failure of the peer review process can be damaging not only to science itself but also to public perception of the scientific community. Examples of such failures have recently come to ight and have provoked public discussion and congressional inquiries.

The scientific community receives vast amounts of public moneys. In our political system, any recipient of public largesse is subject to public scrutiny in the expenditure of those funds. Resistance on the part of the scientific community to such public scrutiny is an inappropriate response and betrays a basic misunderstanding of the trust with which public moneys

are always impressed.

The peer review process is scientific research, and especially in biomedical research, plays a key role not only in determining what gets published, but even earlier in determining what research is to be funded by public moneys. The peer review process is inextricably linked to the expenditure of public funds for scientific research because government funding agencies, such as the National Institutes of Health (NIH) have limited ability to pass judgment on what research should be undertaken and funded and therefore rely on judgments from the peer review process. Peer review is, therefore, fairly subject to public examination to determine if it adequately performs its crucial role. To date, the public inquiry has focused on the conduct of particular investigators and has only peripherally touched the question of why the peer review process did not succeed in its quality-control function in those few publicly discussed cases. We think it is time that the peer review process itself becomes a focus of that public examination.

In this report, we present an actual case in which we have been involved for the past 3 years that deals with the expenditure of large amounts of public funds and with publication decisions by leading medical journals. [1] We believe that this particular case is valuable to the current debate on the peer review process because it offers a multitude of issues, the discussion and debate of which may benefit the scientific community. We will offer our own comments, but with the acknowledment that our involvement in this particular case, which is ongoing, could fairly be said to blunt our objectivity.

THE CASE

Prior to September 1986, one of us (E.I.C.) was the director of research of the Department of Pediatric Otolaryngology of Children's Hospital of Pittsburgh, Pa, and a professor of otolaryngology of the School of Medicine of the University of Pittsburgh. Children's Hospital of Pittsburgh houses an NIH-funded program project known as the "Otitis Media Research Center" (OMRC), staffed by faculty members from the University of Pittsburgh. Charles D. Bluestone, MD, was and is the principal investigator for the program project. The OMRC was initially funded in 1980 and has been the recipient of $18.5 million of NIH funding to the present date.

The OMRC was also the recipient of substantial funding ($3.5 million) from private pharmaceutical companies to perform clinical trials and laboratory studies to determine the effectiveness of various new antimicrobials. [2] In 1983, E.I.C., concerned over the ethics of mixing publich and private funding in the OMRC's research into antibiotic effectiveness, refused to participate further in privately funded clinical trials. The OMRC, in filing grant renewal applications with the NIH, did not disclose the presence of substantial pharmaceutical company funding received by the OMRC for research on antibiotic effectiveness. [3]

The NIH grant to the OMRC included funding for several clinical trials to determine the efficacy of various treatment options for otitis media. One such study (AB-OME-I) was a double-blind, placebo-controlled, randomized clinical trial to determine the efficacy of amoxicillin (Amoxil) with and without decongestant/antihistamine in the treatment of persistent asymptomatic middle-ear effusions in infants and children. The study, as described in the protocol, had a sample size estimate of 1040 subjects. Dr Bluestone was the principal investigator for this study. E.I.C. was listed in the grant as "co-principal investigator" for AB-OME-I.

In 1984, the AB-OME-I clinical trial was terminated by the OMRC with half the target sample size (518 patients). A new clinical trial, AB-OME-II, was then initiated that continued the AB-OME-I protocol but added two additional antimicrobials (cefaclor [Ceclor' and Pediazole). Funding from two pharmaceutical companies was obtained for AB-OME-II without the knowledge of the NIH, which continued to fund AB-OME-II under the NIH program project grant. [4]

In 1985, OMRC staff undertook analysis and reporting of the AB-OME-I study data and circulated drafts arguing that the drug amoxicillin was shown by the study data to be effective. E.I.C. disagreed with the OMRC's interpretation and circulated memoranda within the OMRC that criticized the draft reports of AB-OME-I. [5] Internal debate within the OMRC did not

succeed in resolving the differences between E.I.C. and other OMRC personnel.

In May 1986, the OMRC sent its report of the AB-OME-I study to The New England Journal of Medicine (NEJM), asserting that amoxicillin had been shown to be effective in the treatment of secretory otitis media in children. [6] Unable to persuade the rest of the OMRC staff to accept his views, E.I.C. then drafted his own analysis of the AB-OME-I, enlisting another of us (T.W.M.) and a graduate student for assistance in statistical analysis. With prior notification to the principal investigator of the OMRC and the chairman of the Department of Otolaryngology, E.I.C. and coauthors sent their manuscript to the NEJM under a cover letter in which (1) E.I.C. referred to the OMRC manuscript submitted the month before by the OMRC, (2) E.I.C. stated that he was a coinvestigator who had not succeeded in persuaading the authors of the OMRC manuscript to take his concerns into account, and (3) E.I.C. stated that "our analyses of the same data yield considerably more ambiguous results."

The chairman of the Department of Otolargyngology and the medical director of Children's Hospital of Pittsburgh then demanded that E.I.C. withdraw his manuscript from the NEJM. E.I.C. refused. On request of the editor of the NEJM, the medical director of Children's Hospital of Pittsburgh and the chairman of the Department of Otaolargyngology wrote to the NEJM and stated that the OMRC manuscript was "authorized" and that E.I.C. had acted "without authority" and his "actions were, and continue to be, unethical, improper and a source of great academic concern." [7] The NEJM then returned E.I.C.'s manuscript without submitting it to peer review. [8] In February 1987 the NEJM published the OMRC article, which argued that amoxicillin is efficacious in the treatment of secretory otitis media in children. [9] The NEJM, in publishing the OMRC article, did not inform its readership that it had received a manuscript from a coinvestigator of the OMRC that reached contrary results and did not invite E.I.C. to summarize his disagreements in a letter to the editor to be published simultaneously.

E.I.C. was then dismissed from his position as director of research and isolated from his colleagues.

E.I.C. requested from the chairman of his department permission to publish his dissenting analysis of the AB-OME-I data after the publication of the OMRC article in the NEJM. In April 1987, the chairman granted such permission. E.I.C. then submitted his manuscript firs to The Lancet, then to JAMA. In each case, E.I.C. in a cover letter explained that his manuscript was in the nature of a dissent and enclosed a reprint of the OMRC article published in the NEJM. In September 1987, E.I.C. presented his dissenting views at the annual meeting of the American Academy of Otolaryngology, again citing the OMRC publication in the NEJM and comparing his analysis with the conclusions of the OMRC.

JAMA submitted E.I.C.'s dissenting manuscript to external peer review. In September 1987, the editor of JAMA expressed an interest in publishing E.I.C.'s dissenting views and enclosed the comments of the external reviewers.

In November 1987, one of the reviewers to whom JAMA had submitted E.I.C.'s manuscript wrote to Charles Bluestone, MD, and enclosed his review (highly critical) of E.I.C.'s manuscript. This reviewer was the chairman of the NIH site-visit team that had approved the original program project grant for the OMRC. Furthermore, Dr Bluestone, principal investigator of the OMRC grant, had also served as the external consultant on an NIH-funded grant to the reviewer. This reviewer suggested to Dr Bluestone that Bluestone should obtain a copy of E.I.C.'s manuscript from JAMA.

In December 1987, E.I.C. and his coauthors revised the dissenting manuscript. In January 1988, JAMA sent the revised E.I.C. manuscript to Dr Bluestone. Dr Bluestone returned the revised E.I.C. manuscript to JAMA, declining to review it and citing his conflict of interest as a coauthor of the NEJM article. One week thereafter, the chairman of the Department of Otolaryngology filed written charges with the dean of the medical school, alleging that E.I.C. had committed "research misconduct."

The dean of the medical school appointed an ad hoc review committee, which filed a report in March 1988 in which it found that E.I.C. had

committed "an inappropriate expropriation of data [of the AB-OME-I study]" that constituted a "serious breach of research integrity." E.I.C. immediately forwarded to JAMA a copy of the report of this ad hoc committee.

In May 1988, the editor of JAMA declined to publish E.I.C.'s revised dissent, citing three factors: (1) possible lack of availability of the AB-OME-I data, (2) pending "research misconduct" charges against E.I.C. at the University of Pittsburgh, and (3) pending NIH investigation.

The dean of the medical school then appointed a hearing board within the medical school, which found that E.I.C. had committed" serious violations of research ethics" and had "fraudulently present[ed] the data [from AB-OME-I] as if he was responsible for it."[10] In April 1989, the dean of the School of Medicine accepted the conclusion of the hearing board. E.I.C. then appealed the finding to the senior vice president for the health sciences (Dr Thomas Detre), who affirmed it on the basis that E.I.C. had "misuse[d] data over which he had no authority...." E.I.C. has subsequently appealed that decision to the president of the University of Pittsburgh, who has in turn appointed an appeals panel of professors outside the School of Medicine.

QUESTIONS PRESENTED

We suggest that the following questions are presented by the case we have described:

1. Does a coinvestigator or coresearcher have the right to dissent from the analysis of the principal investigator and to utilize the data generated by the publicly funded research in that dissent?

2. If so, when and how may the dissenting coresearcher exercise the right of dissent?

3. May the principal investigator or the grantee institution take punitive action against the dissenting coresearcher if he or she insists on making his or her analysis available to the journal to which the principal investigator has submitted the manuscript?

3. Should a scholarly journal, in receipt of the analysis of the principal investigator and of the dissenting analysis of the coresearcher, heed an attempt by the principal investigator or the grantee institution to label the dissent "unauthorized"?

4. Should a scholarly journal deprive external reviewers of the knowledge that a dissenting analysis exists and has been received and fail to provide the reviewers with that dissenting analysis? Should that scholarly journal publish the manuscript of the principla investigator without simultaneously informing its readership that it received a dissenting analysis from a coresearcher and without affording that dissenting coresearcher the opportunity to express a dissenting analysis in some form (letter to the editor, condensed postscript, etc)?

5. Should a scholarly journal refuse to publish the dissenting views of the coresearcher not because of scientific inadequacies in his or her analysis but because the dissenter has become enmeshed in controversy and because the grantee institution has taken actions against the dissenter?

6. Is the peer review process adequately equipped to detect undisclosed conflicts of interest or undisclosed problems with the conduct of studies and with the analysis of data (and therefore does not need to be provided with dissenting views of coresearchers)?

COMMENT

We think it self-evident that a coresearcher or coinvestigator must have the right to make a dissenting analysis of data generated by a publicly funded study. In the case we have described, the Tenure and Academic Freedom Committee of the Faculty Senate obtained an opinion from a professor of law who specializes in the field of intellectual property. Her opinion was that there is a right of dissent and a corresponding right to utilize data from a publicly funded study. Although ad hoc committees or senior officers have asserted that the grantee insitution has the right to control the data and to prohibit dissent based on those data, no such committee or senior officer has ever produced any legal research or analysis to support such an opinion. It would seem that the data generated by a publicly funded study ultimately are public property and that the right to use such data or to make a dissenting analysis is not even tied to

the scientist's having originally been part of the research team. [11] It is hard to imagine public acceptance in the United States of a theory of private ownership of data generated by publicly funded studies that could be used to prohibit dissent. We leave it to those who assert such a right to justify it with policy arguments and law. [12]

The timing of dissent may, however, be a more significant question. The principal investigator obviously must have a right to pubish his or her analysis in advance of any dissenting analysis, provided, of course, that the results are submitted for publication in timely fashion. The right to publish dissent implies, we think, that the views of the principal investigator have been published. We do not argue otherwise.

The existence of the dissenting views should, we think, be made known to the external reviewers of the scholarly journal to which the principal investigator has submitted his work, and their contents made available to them. The peer review process can best function, we suggest, when reviewers are equipped not only with the analysis of the principal investigator, but also with the criticisms and contrary analysis of the dissenting coresearcher. Not only should dissenting coresearchers have the right to make their views known to the journal to which the principal investigators have submitted their manuscript, but the principal investigators ought to be obligated to make known to that journal that there is a dissenting analysis within their own research team. Again, we find it difficult to conceive of a policy argument that the peer review process is harmed and not benefited by having available to it the dissenting view. In this country, we as a people believe that truth will emerge from spirited debate and the availability of competing views. We leave it to those who believe otherwise to explain how or why the peer review process could possibly be harmed by the availability of dissent.

It follows that the editor of the NEJM should not have heeded the attempt by the principal investigator and the grantee institution to label the dissenting views "unauthorized." The NEJM should not have failed to make available to the external reviewers full knowledge of the fact of dissent and should have provided to the reviewers the dissenting manuscript. It is a breach of trust, we think, for the editor of the NEJM to have asked the external reviewers to comment only ont he principal investigator's manuscript without having available the views of the dissenting coresearcher. [13] Similarly, the NEJM, in publishing the manuscript of the principal investigator, should be obligated to inform its readership of the existence of dissent and of the arguments of the dissenter.

When the dissenting coinvestigator submitted his manuscript to a second scholarly journal, that journal, we suggest, should have made its decision to publish or not publish purely on scientific grounds, just as the original journal, in publishing the principal investigator's analysis, should have made its decision purely on scientific grounds. By not publishing the dissent because of the possible unavailability of the study data and the pendency of misconduct charges against the dissenting coresearcher, the journal in effect defeated the peer review process and allowed a veto power to the principal investigator and the grantee institution.

The editorial staff of a journal and the journal's external reviewers have a difficult job. They are presented with a single manuscript that rarely discloses the underlying details and problmes. Authors do not generally disclose how they have handled debatable questions of data organization or analysis or how they have reached consensus among the authors. It is physically and logically impossible to expect that the journals and reviewers perform an oversight function for the integrity of the research being reviewed. In the present case, for example, the NEJM and its reviewers were not informed that the study had been stopped early, at half the contemplated sample size, that the principal investigator was in possession of further clinical data (from the AB-OME-II study) that did not show a statistically significant difference between placebo and amoxicillin treatment arms, and that the AB-OME-I placebo cure rate (14%) was significantly less than that encountered in an earlier study at the OMRC (24%) and that encountered in AB-OME-II (27%). [14] Further, the reviewers

were not informed that the center that had submitted the manuscript received millions of dollars of support from the pharmaceutical industry. [15] The journal and its reviewers cannot be faulted for not knowing these facts.

Reviewers for a journal are judges of the quality and correctness of the manuscript they review. No reasonable judge would claim that he or she can decide cases by reading the submissions of only one party. When a contrary brief is available, fairness and the search for truth require that the judge read both briefs. There can be no doubt that those journals that have in the past published articles by authors who were subsequently shown to have fraudulently created data or misrepresented results would have been well served had a contemporaneous dissent or critique been available and read.

Dissent in biomedical research publication is not common. Given our experience in this case, it is likely to become, if anything, even rarer. We think this is unfortunate and disserves science and the public. The near-total dependence on major finding to support biomedical research makes it unlikely that a dissenting coresearcher can acquire a new source of funding to perform the same, earlier-funded research to produce data to support a dissent. When American taxpayers provide millions of dollars to perform a study such as AB-OME-I, the study is not likely to be repeated. If members of the team that performed the expensive, once-in-a-lifetime clinical trial do not agree with the principal investigator's analysis, they must have the right to publish their dissenting views, and they must be encouraged to publish their dissenting views.

The peer review process functions best when it focuses exclusively on questions of scientific validity and merit and when it encompasses all information available on those questions, including the views of dissenting researchers when available. The peer review process risks failure when it blinds itself to allinformation available on scientific validity or when it enmeshes itself in concepts of "authorization," "data ownership," and misconduct charges against dissenting coinvestigators.

The practical costs of a failure of peer review can be large. In this case, for example, the American public spends on the order of $2 billion annually on the treatment of otitis media. A scientifically defensible argument that antimicrobials are not effective in the treatment of otitis media in children could, if accepted, save the American public millions of dollars every year. The peer review process has failed when issues not properly part of that process have prevented the publication of that analysis.

We believe that the case presented is a unique opportunity to examine how the peer review process should deal with dissent in scientific publication. Science, in its long history, has never been served by the suppression of dissent. Every instance in which scientific argument has been suppressed in the name of unanimity or some passing orthodoxy has invariably proved to be an embarrassment. The current peer review system, however, as shown by this case, proved to be unprepated to embrace dissent and use it to optimize the peer review process to the benefit of science and the public.

Notes

[1.] The case presented has already been the subject of considerable discussion. For a partial bibliography to date, please see the following: "Bitter Dispute Reaches NIH" (Nature. 1989>340:668-669)> "Congressional Inquiry Into Allegations of Scientific Fraud" (Lancet. 1989>2:38)> "Dispute May Clarify Who Owns the Results Produced From Federally Supported Studies" (Chronicle Higher Educ. June 28, 1989>35:A4)> "Business and Scholarship: A New Ethical Quandary" (New York Times. June 12, 1989:1)> "Congress Probes Research Funding: Treatment for Ear Infections Is Subject of Disputed Medical Study" (Washington Post. June 13, 1989:9 [Health Section])> "Medical Research and Business: Pass the Whistle, Nurse" (Economist. October 8-14, 1988:86)> "Science and Technology: Policing the Page" (Economist. June 3, 1989:83)> "Should Journal Editors Play Science Cops?" (Scientist. October 31, 1988:3)> "Corporate-funded Research May Be Hazardous to Your Health" (Bull Atomic Scientists. April, 1989:32)> "Research Ethics: Testing, Investing" (New York Newsday. December 6, 1988:3

[Discovery Section])> "Pitt Prof Questions Validity of Study Funded by Drug Firms" (Pittsburgh Press. September 29, 1988:1)> and "Conflicts in Medical Research Cited" (Pittsburgh Press. June 14, 1989:A18).

The Government Operations Committee's Sub-committee on Human Resources and Intergovernmental Relations has conducted 2 days of public hearings devoted in part to this controversy. See Federal Response to Misconduct in Science: Are Conflicts of Interest Hazardous to Our Health? Hearing Before a Subcommittee of the House Committee on Government Operations, 100th Cong, 2nd Sess (September 29, 1988) (hereinafter Hearings, September 1988)> and Is Science for Sale? Conflicts of Interest vs the Public Interest: Hearing Before a Subcommittee of the House Committee on Government Operations, 101st Cong, 1st Sess (June 13, 1989) (hereinafter Hearings, June 1989).

[2.] See "Congressional Inquiry Into Allegations of Scientific Fraud" (Lancet. 1989>2:38).

[3.] See Hearings, September 1988, pp 258-272.

[4.] See als Hearings, September 1988, pp 243-249. (The principal investigator explained to the National Institutes of Health [NIH] after the fact that the failure to have informed the NIH of the addition of two additional antibiotics and private funding to the NIH-funded study was "an omission.")

[5.] It is not possible in the context of this report to review in detail the points of contention between the Otitis Media Research Center (OMRC) and E.I.C. The disagreements with respect to analysis of AB-OME-I data were principally the following:

Whether otoscopic measurements made during the study were infected with a systematic error such that they should not be included in the disease definition. E.I.C. contended that there was strong statistical evidence of systematic bias in the otoscopic measurements and that analysis should be done using tympanometry. Using tympanometry, it is indisputable that the AB-OME-I data show no drug efficacy. The OMRC used otoscopic measurements and scored ears as having no effusion on the basis of a negative otoscopy reading, whereas the corresponding tympanometry measurement suggested a high probability of effusion.

Whether the end point for analysis of antibiotic efficacy should be 4 weeks or whether it should be based on 2-week, 4-week, and 8-week data. E.I.C. argued that there was a strong recurrence pattern in the antibiotic-treated group that made it improper to limit the analysis to a single time point of 4 weeks or less.

Whether correction for differences in prognostic factors at study input was necessary as between the placebo group and the amoxicillin groups. E.I.C. found that correction was necessary. The OMRC made no such correction. See also Hearings, June 1989, pp 307-308.

[6.] As originally submitted to The New England Journal of Medicine (NEJM), the OMRC manuscript argued that amoxicillin was effective and should be used in the treatment of secretory otitis media in children. After external review, the NEJM required the conclusion to be toned down to an assertion that the drug was marginally effective.

[7.] See Hearings, September 1988, pp 253-255.

[8.] In November 1986, the NEJM accepted the OMRC paper and requested a "conflict of interest" disclosure. The OMRC failed to inform the NEJM, as it had failed to inform the NIH, that it had received millions of dollars in funding from the manufacturers of antibiotics, including manufacturers of amoxicillin.

[9.] Mandel EM, Rockette HE, Bluestone CD, Paradise JL, Nozza RJ. Efficacy of amoxicillin with and without decongestant-antihistamine for otitis media with effusion in children: results of a double-blind, randomized trial. N Engl J Med. 1987>316:432-437.

[10.] The basis for this finding was that E.I.C., when he wrote his analysis of the AB-OME-I data within the OMRC, drafted a manuscript that simply analyzed the data and announced conclusions. E.I.C., in sending that manuscript first to the NEJM, then to The Lancet and JAMA, always included a cover letter explaining that the manuscript was in the nature of a dissent. There has never been any indication that any recipient of E.I.C.'s manuscript and the accompanying cover letters failed to understand that the

manuscript was a dissent. See also Hearings, June 1989, pp 251-253.

[11.] The editorial board of JAMA has taken the position that data generated by publicly funded studies are public property. See remarks of George D. Lundberg, MD, editor of JAMA, made before the Human Resources and Intergovernmental Relations Subcommittee of the House Committee on Government Operations. See Hearings, June 1989, p 33).

[12.] For a discussion of the data ownership issue, see "Dispute May Clarify Who Owns the Results Produced From Federally Supported Studies" (Chronicle Higher Educ. June 28, 1989>35:A4)> "Bitter Dispute Reaches NIH" (Nature. 1989>340:668-669) ("Who owns the data?").

[13.] The decision of the editor of the NEJM to return E.I.C.'s manuscript unreviewed is criticized in "Should Journal Editors Play Science Cops?" (Scientist. October 31, 1988:3).

[14.] For a discussion of the AB-OME-II data, see Hearings, June 1989, pp 214-217.

[15.] See Hearings, June 1989, pp 272-273.

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Times change and so does the JSBM: An editorial
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ABSTRACT: An editorial discusses the changes being made to the Journal of Small Business Management.

TEXT: Small business research is an evolving field. Twenty-five years ago, the field was in its infancy; small business was barely considered an academic discipline, and those teaching it were not in the mainstream. which focussed on larger firms. There were few small business research journals, and how-to and descriptive work dominated the available publications, most of it in LI.S. settings.

All that has changed. In a rapidly changing and globalized business world the returns-to-scale advantages of the large firm are offset hy the flexibility small firms can exercise in meeting consumer demand. The study of small(ly' Of business has become a core requirement in many business schools and well-recognized in others. Numerous new acad.emic journals have appeared, particularly since 1986. and research has evolved from the exploratory* descriptive stage of the scientific process to hypothesis-testing using sophisticated statistical techniques. Further, research has become much more broad-based, with important contributions coming from professors outside the U.S.

Founded in 1963, the Jo)rt nl al of Small Businesstess MVaManagementhas spanned these changes. This issue marks the continuing evolution of the Journal with the estahlishment of three important alterations: the consideration of "Longer Articles" for publication, the implementation of new procedures for review, and an increased focus on international issues in small business. Many of these changes are described in the revised "Guidelines for Authors" that appears in this issue.

Consideration of "Longer Articles" reflects the increasing sophistication of research in small business. The typical JSBM research paper has been of moderate length, using well-known statistical techniques to address its research questions and readable by most small business researchers. While very useful, this format can be confining to authors who seek the full exploration of complex research questions, frequently aimed at specialized segments of the research community. To reflect the growing sophistication of small business research and the need for this kind of in-depth discussion, theJSBMMT will now consider these more lengthy manuscripts, as well its traditional manuscripts. However Longer Articles must have merit in keeping with their length.

The second change concerns the assignment of reviewers to manuscripts and reflects the increasingly specialized focus of many small business researchers (including the distinguished scholars that make up our editorial board). Instead of researching small business generally many now focus more narrowly on "small business organizational structure "small business finance or similar subspecializations. We increasingly find reviewers unable to review manuscripts outside of their particular preferred subspecialization,. Consequently, we will be assigning manuscripts to reviewers based on their area(s) of expertise. To facilitate this process, authors submitting manuscripts are required to state the

subspecializations that their article addresses, as well aswhether it is to be considered as a Longer Article, Regular Article, or Global Perspectives piece.

The final change addresses the broad and international character of modern small business research. The Journal will no longer publish Book Reviews or "Small Business News and Views" pieces, devoting this space instead to our expanded Global Perspectives section (formerly called "International Notes"). This editorially-reviewed section will contain shorter, spirited pieces that describe and explore new trends in small business worldwide.

The only constant is constant change. The danger of change is faddishness, and research publications can be just as prone to being enamored with the glamorous and temporary as the popular press. However, the trends in small business research are reasonably clear, and it is time for the JSBM to reflect them.

Fred Scherr

Editor, Journal of Small Business Management

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